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REMARKS

ON THE

INTRODUCTION OF DIPHTHERIA INTO VICTORIA.

BY

WILLIAM THOMSON, F.R.C.S. ED.

"HE WHO DESIRES TO SERVE THE INTERESTS OF SCIENCE AND HOPES TO EXTEND THE
BOUNDS OF NATURAL KNOWLEDGE, SHOULD ENDEAVOUR TO PRESERVE HIS
INDIVIDUALITY, DEFEND LIBERTY OF OPINION, AND STRIVE TO RETAIN
COMPLETE INDEPENDENCE OF THOUGHT AND ACTION."

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“ Minute investigation in connexion with disease has been most unwisely discouraged, by purely scientific men on the one hand, and by those who confine themselves to their practical medical duties on the other. By the first, because they think that medical practice affords occupation enough for one man ; by the last, on the ground that scientific work unfits a man for the practical duties. It has too often happened that the very few who have devoted themselves to real medical enquiry, have been unfairly treated, and by the very persons who ought to have afforded them support. The time has now arrived when the incentives to this course should be openly condemned, as resulting from narrow ancient prejudice, which has long survived its allotted term. Every intelligent person will do his utmost to further those branches of investigation which have already exerted so great an influence upon the discovery of the wonderful changes which occur in man's body in health and disease, and therefore upon the progress of medicine.”

From the Australian Medical Journal, July 1872.

REMARKS ON THE INTRODUCTION OF DIPHTHERIA INTO VICTORIA.

By WILLIAM THOMSON, F.R.C.S. Ed.

It is often said with truth that few places are more favourable than this for watching the origin of zymotic fevers. It can only be reached from the great centres of old populations by a long sea voyage, during which latent fevers have time to ripen. Thinly inhabited, the source of any unusual form of fever can readily be ascertained, whether independent of external intercourse or brought into the country by new-comers. With these peculiar advantages for testing the validity of the doctrines of the nature and origin of zymotic disease, it would be to the credit of all if they could be employed for that purpose. At the present moment three different inquiries of the kind are going on, two of them by Royal Commissions, whose reports are about to be laid before Parliament. After fourteen years presence of diphtheria, there is an inquiry into its origin. More promptly the cause of the sudden appearance of vesicular epizootic aphtha was at once investigated. The source of the small-pox is too obvious to require explanation beyond a statement of the fact that it was brought by passengers in the mail steamer from America. It is needless anticipating the conclusion at which the commission on aphtha may arrive on the origin of that cattle fever, but there is no reason why the probable origin of diphtheria may not be independently considered. Some months ago

the writer addressed a communication to a contemporary on this subject, indicating some of the possible sources of zymotic diseases, yet acknowledging the difficulties besetting the inquiry. Referring to the topic, the *Lancet*,* after reminding us of our peculiar opportunities, observes that medical men practising here would render an essential service to science if they would carefully investigate the first appearance of zymotic diseases in isolated houses, adding that, "It is under such conditions alone that we may expect to obtain satisfactory proof of the spontaneous origin of infectious poisons, and we shall be glad to receive information from such sources." A knowledge of the cause and nature of zymotic disease is the basis of public sanitary work, and it is a duty if possible to add to it. The following additional particulars being in parts somewhat technical, are perhaps more suited for a purely medical publication than the general remarks contributed on the previous occasion.

However desirable it would be to premise some general account of the various theoretical views now held on the nature of zymotic action, so as to connect it with its causation, it would be necessarily so brief and cursory that it will be better deferred to some future time, when it may be given at greater length and in detail in connection with questions of prophylaxis and the supposed efficacy of (so-called) disinfectants. The various parasitic and germ theories, and the catalytic theories, including the latest of them, known as the correlation of zymotic diseases—all as wide apart as the vital and the physical doctrines of life—have direct bearing upon the subject, hypothetical it may be and conjectural, but suggestive of experiment and useful purpose. Those theories first originated the idea of specific fever poisons and contagious entities, whose existence it is the object of some later investigations to disprove. Whether those contagia are particles thrown off from diseased bodies to affect similarly other healthy yet susceptible bodies, and descending like the bodies they infect from one generation to another; or fungi, each the cause of a special fever; or living organisms of the lowest forms of animal life, capable of rapidly

* See note, page 28.

multiplying themselves within the blood and tissues ; or some form of degenerated bioplasm capable of imparting its mode to other bioplasm ; or non-living albuminoid, ferment or catalytic ; or organism generated *de novo* by isomeric re-arrangement of molecules of inorganic matter ; or whether there are no such germs or contagia or particles at all, are all moot points connected with the nature and origin of zymotic fevers ; but certainly not on this occasion open for discussion. Of one only, the latest enunciated, need a word be said.* It is that which asserts that decaying organic matter in contact with living organisms confers its disintegrating dying molecular action, setting up destructive inflammation, which is the essence of all fever. The cause being always the same, varies in effect according to the tissue in which it is implanted. When applied to the skin, small-pox results ; when to the organic nerves, cholera ; to the blood, typhus ; to the glands of the intestines, typhoid ; to uterine surfaces, puerperal fever, &c. This is likely to become a very popular doctrine, and if true the theory of zymosis and the practice of sanitation will be simplified. But, like the rest, it is not free from difficulties. It affirms that a small-pox pustule differs in no perceptible way from the pustule of croton oil or tartar emetic. In external appearance they are alike ; but if Hallier be correct, the pus in the one contains bacteria, in the other none. But whether or not, there is still the physiological test. Inoculation by the one pus would produce small-pox ; by the other, only a local irritation. This test alone seems fatal to the theory, and strengthens other proofs of a specific virus in whatever form it may exist as an entity. On this point the experiments of Professor Chauvaux were conclusive. In addition to these references, the laws of endemic, epidemic, and pandemic conditions, as well as the laws of contagious, infectious and enthetic poisons, would require consideration. Although allusions to these must frequently be made while illustrating the special subject, yet an acquaintance with them must be taken for granted, for a full discussion of them would be out of the scope of a brief review of a single variety.

* Zymotic Diseases, their Correlation and Causation. By A. Wolff, 1872.

Such are a few of the preliminary points in all inquiries into the origin and nature of zymotic fevers. While undecided they may be studied in conjunction with facts forced upon attention by individual cases of disease. Few have skill, and fewer opportunity to employ the higher methods of research ; yet all who are daily brought face to face with disease must form some notion of its nature and origin. Men of leisure, trained skill, working in centres of scientific culture and refined philosophical thought, in laboratories replete with apparatus, are, after prolonged research, unable to determine whether a living organism is a cause or a consequence of fever, of fermentation, or of putrefaction ; essential to either process, or only incidental or causal. Where philosophers fail, physicians, who dare not pursue similar experimental methods, need no apology.

From the briefest outline of the leading doctrines of the causation of zymotic fevers, the first inquiry is, Have those fevers always been imported ? or have any appeared spontaneously ? Did scarlatina, measles, varicella, hooping-cough, small-pox, diphtheria, typhoid, typhus, arise *de novo* ? Or, were the germs of each exanthem or other fever, the virus, poison, contagium, *materies morbi*, brought hither ? Was epizootic pleuro-pneumonia (not the sporadic form, so often seen and mistaken for the contagious disease, just as sporadic cholera has been pronounced the graver malady)—was it imported or engendered from local indigenous causes ? Was the vesicular aphtha brought on shore ? Apart from science altogether, shrewd men believe in contagion, and, doubting spontaneous theories, urge quarantine as the safeguard : that is, they would prevent from entering Port Phillip, a disease which some say originates on Keilor Plains ! Though “ their talk is of bullocks,” it is wiser than words often spoken of men. Will cholera poison, be it germ, bacterium, spore, sporidium, psorosperm, microzyme, urocystis, spring anew, or come in the bowels of some infected host, nurturing the cause to maturity after partaking of diseased rice or polluted water in India ? Will any advocate of the spontaneous theory display synthetic skill by naming beforehand conditions amidst which its origin may be predicated ? Whoever cares to bestow a few moments upon questions of both scientific interest and practical concern may not object

to the desultory remarks of one to whom they have recently been suggestive.

It would be frivolous to allude to the obvious source of the ordinary exanthems, for they have been re-introduced a thousand times, by infected vessels. It is needless to the point in debate to convince even those ever prone to encumber every medical question with difficulty. They are too conversant with the fact to be dogmatic over the theory. When an obvious source of imported small-pox stares everybody in the face, it will not be attributed to cosmic influence. The same may be said of carried typhus, as in a memorable voyage cited by Mr. Herbert Spencer,* who erroneously inferred the fever was engendered on board an overcrowded ship, whereas it was brought direct from the fever beds of Glasgow. Of this fact the writer, who was on board, had absolute demonstration, when the first case appeared while the vessel still lay in the Mersey, the fever spreading from patient to patient, but making no simultaneous outburst amongst a number. There was at one time one hundred and eighty children ill of hooping-cough in the same vessel, the disease spreading through three hundred children on board, from one child embarked convalescing. The history of that voyage alone, explains how contagion is transported from old to young countries, giving to these an evil they do not themselves get rid of. It also proves that those who have not passed through the crisis of typhus, are still as susceptible as if they remained in fever-stricken cities. Their fermentible elements await the ferment. Whether new conditions generate this ferment, or whether it is always transported, is still an unsolved problem. There have been some marked opportunities for observing the introduction of contagious diseases into this colony; and if all the facts were carefully collated, they would illustrate the general question. The instances usually created much anxiety, often panic and alarm, and the very ships were easily traced. There has never yet been undeniable proof of a spontaneous outbreak of any one contagious fever in the country. The keen controversy on the introduction of epizootic pleuro-pneumonia must be in every one's

* *Essays: Scientific, etc.*, Longman, 1858.

recollection ; and none can have forgotten the warm discussions on two occasions about small-pox. Although a vessel was placed in quarantine, and a passenger from it infected others, with several fatal cases, yet, because in some instances trace was lost, the disease was insisted to be of spontaneous and not of imported origin. In all instances of doubt, the fever had first attacked a new comer ! Cholera will do the same. So likewise will cattle typhus. If the specific contagions can be excluded, no man can state the conditions under which either will arise independently of them.

In reference to diphtheria the question of first origin has scarcely ever been raised, far less debated. Whether raising it now will lead to practical result is doubted. Still as part of a large subject it may be discussed. The origin of the disease from products of organic decompositions, amidst bad smells, damp soils, and every sort of insanitary surrounding, is often asserted. If this were certain, the knowledge would be, in a hygienic respect, better than the highest skill in empirical methods of treatment ; but it is not certain, and the question of treatment remains a paramount subject of study.

In investigating the very obscure questions of the cause and nature of diphtheria so as to ascertain if possible its probable origin, it must be stated that the month of October, 1858, is the date of the first recorded fatal case. There is said to have been a prior case in a member of the same family. There are also some dubious cases before that date, recorded under other terms ; and it is also supposed that many cases returned as croup were cases of diphtheria, the distinction now drawn between croup and diphtheritic croup not being then so clearly defined as it has lately been. Careful scrutiny of those cases leaves some doubt about their exact pathology. It is scarcely credible that any large proportion of croup were really instances of diphtheria undiagnosed ; the well marked symptoms at the onset of diphtheria could hardly have been so frequently overlooked. From October 20, 1858, till the end of the year there were in the whole colony only six deaths recorded ; in the year 1859, 280 ; and in 1860, there were 636, with still a large number from croup, as shown by the following tabular statement :—

VICTORIA.—*Number of Deaths from the following Causes in each year from 1853 to 1871, both inclusive.*

YEARS.	NUMBER OF DEATHS FROM				
	Measles.	Scarlatina.	Diphtheria.	Croup.	Infantile Diarrhoea (under 5 years).
1853	123	74	—	32	57
(6 months)					
1854	194	63	—	72	203
1855	2	15	—	53	224
1856	6	7	—	69	156
1857	8	25	—	56	342
1858	4	29	6	102	570
1859	2	86	280	229	632
1860	274	427	636	156	772
1861	252	871	510	166	522
1862	20	416	360	134	671
1863	8	312	331	136	525
1864	7	278	451	160	496
1865	11	215	391	171	817
1866	427	462	331	132	966
1867	630	621	334	115	937
1868	24	460	451	194	594
1869	24	224	493	162	811
1870	3	24	418	99	647
1871	4	27	255	90	586

This very sudden increase of diphtheria seems indicative of an epidemic new to the colony. The spreading of this epidemic can be traced from its starting focus gradually almost from door to door and street to street, to suburbs and country, till at length it covers the entire colony, wherein it has remained ever since, making the country a station of the disease, just as it has become a station for epizootic pleuro-pneumonia, and as it soon would have been for vesicular aphtha and small-pox had not sharp measures of destruction of the virus been at once adopted. Statistics clearly indicate its line of march. On 2nd October, a death took place, the first, in King street; on the 17th, the second, close by in Spencer street; on the 21st, the third, in Spring street; on 2nd December, the fourth, in Little Lonsdale street; on the 20th, the fifth, also in Little Lonsdale street; on the 30th, the sixth, in Little Bourke street. On January 1st, 1859, the first

death in a country district occurred at Kyneton. All this took place unnoticed, and without the light of statistical information we should still be in the dark.

Had this disease arisen spontaneously from atmospheric or terrene causes, or from decomposing organic matters, the malaria would have been of more general influence, producing many cases simultaneously. Instead of this there was steady propagation from person to person. That it had no meteorologic origin is made probable by the absence of any electric, ozonic, or other disturbance, as far as can be ascertained by searching the records of the Observatory, under the guidance of Mr. Ellery, our Government astronomer. Dr. Day, of Geelong, assures us that a manifest increase of ozone has always been noted by him concurrently with prevailing excess of diphtheria; but the writer is not aware if his observations extend as far back as to the period immediately before the advent of the epidemic referred to.

But in reference to causation a careful inspection of the table at page 9 cannot fail to show the sudden increase of other zymotic diseases at the same period, and in those especially supposed by many to be closely allied to diphtheria. Thus croup rose from 56 in 1857, to 229 in 1859, while scarlatina went up from 29 deaths to 427, during the same period, and in the year following to the large number of 871. A sudden rise in the number of deaths from infantile diarrhoea took place at the same period, while the deaths from measles were also numerous, although more fluctuating. These details certainly denote some epidemic influence, for although there was no registration of sickness, the mortality indicates an excess. On the correlation theory, it would be assumed that there had long accumulated an intensified virulence from extensive decomposition of organic matters, giving rise to one or other form of zymotic disease according to the tissue or organs attacked in different individuals who fell victims. On the specific poison theory it would require the assumption that about the same time, an importation of different species of germs had taken place simultaneously, an event not impossible, however, as some might think, improbable. The argument for an epidemic constitution of the season may be

strengthened by the occurrence of pleuro-pneumonia at the same period ; for, as already stated, it was in October, 1858, that the first animal was seized with that affection in this colony ; and it would be extremely interesting if statistics of the extent of that fever, known to have been very prevalent and very fatal, could be placed beside those of the other zymotic diseases in the table. And it may not be unworthy of mention that just prior to these outbreaks, the whole poultry in the country were affected by a fatal distemper, spoken of by the late Dr. Maund, in a letter to *The Argus*, as a membranous disease of the throat thought by him to have some analogy to diphtherite. In France, as Trousseau observes, a similar affection among poultry co-existed with epidemic diphtheria. Other affections among the lower animals have been partially associated with diphtheria, but the observations have never been publicly discussed and may have been trustworthy or the reverse. But there is no doubt that blight largely destroyed the vegetable crops, causing great dearth throughout the same period.

All those possible sources of the disease are at best inferential ; and on the contagion theory the evidence is at least equally strong. For it is well known that an epidemic of diphtheria was at its height in England during 1857-8, and that passengers were arriving in ships from British ports almost every week. On board many of those vessels were doubtless numerous persons recently affected, if indeed there were not occasionally some landed while actually suffering from the disease. There was no sort of vigilance ; and judging by what frequently happens in analogous cases, the assumption is by no means unwarrantable. But the records of arrivals of ships and passengers or of quarantine, are devoid of information on the subject. The causes of death in all fatal cases occurring on board ship during the voyages from all ports in the year 1858, that is the year of the outbreak, are given in the health-officer's return, but there is no mention of diphtheria among them.

The details of the table before referred to would be more indicative of some general epidemic influence if they were of constant or even of frequent occurrence ; but they are not, and are perhaps unique and exceptional. Thus, if they be compared with the details of

the outbreak of diphtheria in Philadelphia, it will be found that in that city croup and scarlatina prevailed extensively before the approach of diphtheria, and were therefore irrespective of it. It is only by contrasts and comparisons of this kind that the too common error of drawing a general conclusion from a single instance can be avoided. The following table is for this purpose here reproduced from the treatise of Professors Meigs and Pepper. It shows diphtheria to have appeared in Philadelphia a year later than in Melbourne. In comparing the tables at pages 9 and 12, the columns for measles and infantile diarrhoea may be omitted, when the parallel between the other three columns will appear all the more remarkable.

PHILADELPHIA.—*Number of Deaths from the following Causes in each year from 1855 to 1864, both inclusive.*

YEARS.	NUMBER OF DEATHS FROM		
	Scarlatina.	Diphtheria.	Croup.
1855	163	—	265
1856	992	—	268
1857	704	—	256
1858	241	—	292
1859	232	—	312
1860	206	307	354
1861	329	502	304
1862	461	325	258
1863	275	434	444
1864	349	357	455

In Philadelphia, as in Melbourne, the suddenness of the appearance of diphtheria in the lists of mortality proclaimed the advent of a new disease. There as here some few sporadic cases seem to have been previously noticed, although not with precision enough to arrest attention, or warrant registration of the fact. The argument in favour of an imported source of the disease in Philadelphia is thus equally as conclusive as it is in the history of the epidemic as it first appeared in Melbourne. There seems no other admissible conclusion, unless we resort for another explanation to some such

occult cause as the pandemic wave theory recently propounded by Dr. Lawson.

No systematic effort seems ever to have been made to discover the probable origin of diphtheria, or to trace the mode of its introduction. Several papers appeared in the *Australian Medical Journal*, dealing with the outbreak, but in none of them was anything said on this point. In Mr. Moore's paper, July 1859, the author writing from New Norfolk, in Tasmania, of an epidemic which had occurred there early in that year, after stating that he had not met with a recorded case in the Australian colonies, says, "Diphtheria appears to me to be a complaint proceeding peculiarly from the atmosphere, the blood becomes vitiated by inspiring it." He suggests no possible source of contagion. The disease appeared at New Norfolk about four months later than at Melbourne, frequent intercourse between the places going on meanwhile. In another paper, in October 1859 number of the same journal, by Dr. L. J. Martin, it is stated :—"Almost at the same time that this new disease (Diphtheria) has been committing such ravages and exciting so much anxious attention in the mother country, we have been startled by its mysterious arrival amongst us here at the antipodes, bringing with it all the intractability and fatality which we read of as characterising its progress in England." Here, a mysterious arrival is the assumed source of the malady. In a third paper, in the April 1860 number of the *Journal*, Dr. Hadden opens a paper on Diphtheria with the remark, that "Without stopping here to discuss whether this is a new disease or not ;" and proceeds with observations on the symptoms and treatment in a series of cases, but offers no opinion of the source of the disease itself. In a fourth paper in the same number, Mr. Lempriere alludes to the significance of a single symptom ; and in a fifth paper by Mr. McKenna, the views of Sydenham on epidemic constitutions are given in connexion with the fact that "diphtheria began to show itself" early in 1859 ; but nothing is said about introduction of contagion from without.

In investigating the subject afresh there is therefore slight help obtainable from epidemiological or meteorological writings of the

period, so that there remains, as the only means of now judging whether the disease was imported or of independent origin, the inference from analogy. If the general characters which distinguish it here identify it with the disease of the same name all the world over, the cause must be identical. That these general characters agree there never has been two opinions. About this the whole profession are unanimous. Here, as everywhere else, its specific nature is marked. The inception, premonitory malaise, and fever; the local fauceal exudations; the various complications; the sources of danger; the sequelæ; the effects of treatment; and the contagion, all combine in a precise order of morbid phenomena denoting the one unmodified specific disease. In Victoria, the affection answers exactly to the descriptions given of the same disease in England, America, Europe, Asia. The ancient Syrian and the modern Melbourne ulcer is the same. Time, place, season, never alter it. It is still without preference for social position. Poverty has no predisposing influence; affluence affords no immunity. Marsh or rock, hill-top or valley, sunny side or cloudy, offensive airs or pure atmosphere, bad drains or good ventilation, dirt or cleanliness, squalor or refinement, have no effect in causing mildness or severity. Victims are picked capriciously from all classes, and the specific action goes on through every overlying circumstance.

“Nothing is as yet known with regard to the general conditions which favour this production of diphtheria; and it appears to have prevailed with equal severity in healthy and unhealthy situations; in damp marshy districts and in dry hilly regions; in the crowded filthy houses of great cities, and in sparsely-populated villages; in the depth of winter and in the intense heat of summer.”

“Diphtheria exhibits no preference for any particular social condition. Privation exercises no predisposing influence, persons enjoying affluent or comfortable circumstances not being less liable than others.”

“Villages in marshes in England escaped, while those on high ground suffered.”

“Houses with offensive atmosphere and defective drainage, over-

crowding, bad ventilation, uncleanliness, are not proven to have had worse cases than those free from these evils."

These few extracts from writings of the highest authority attest how little conditions of life disturb the causes of the affection, which must therefore be of specific nature. In whatever physical state the cause exists, as germs entering the blood by inspiration or ingestion, or self-generated within the body under unknown influences acting upon the organic nervous system, that cause seems to act irrespective of any observable surrounding. A very detailed account recently given of the disease as it appeared in Malta serves for a description applicable to Melbourne, or any other part of Victoria. In short, Continental, English, and American writers, in east or west, concur in depicting one unmodified disease, the specificity of which must be undoubted. From its universality it can hardly be a question of greater or less probability whether it has originated here, or has been imported. If it arose from any special error of life, that error must be common over the whole world, and be marked and noticeable. If it be associated with fermenting dejecta or animal exuviae, waste putrefying organic matter, animal or vegetable, they as sources of fever are nothing new, nor have they disappeared along with the partial cessation or abatement of the epidemic. They have always existed, but have not always been potent agencies in generating diphtheria. What then is the new factor? An imported contagium? The first house in which the writer ever saw a case of diphtheria—and he had been many years in practice before he saw or even heard of a case in this country—the house stands after fourteen years in every respect now as it stood then. The drains, outhouses, floors, ceilings, tanks, passages, are unaltered in the least. For fourteen years there has been no change. There early in the year 1859, two fine previously healthy children were suddenly cut off on the first approach of the epidemic. Since that time a large family have been reared in the same house, free from any illness of any kind. No floor has been lifted, no wall taken down, everything remains untouched. If soil exhalations or putrid effluvia caused the only illness, what has spared the other children under precisely the same set of conditions? It is more

probable that a specific poison attacked the two children, and that the rest have not been brought under the power of the same virulence. After careful examination, this seems the only reasonable conclusion. It is often said that diphtheria clings tenaciously to particular houses. It was not so in this instance. On entering a house it is no more apt to pass through every individual in it than other exanthems, measles, or scarlatina, or variola, or even varicella. It is probably less apt to do so than these. Much oftener is only one child in a family attacked with diphtheria than is the case with other exanthems. Neither is it more prone to leave a house poisoned with fomites. The only difference seems to be that people are more familiar with ordinary eruptive fevers than with diphtheria, and give greater prominence to the intractable features of the worst cases. The numbers in the table clearly prove that other so-called simple affections usually of childhood are equally as fatal. These numbers indicate a decline in fatality of all the zymotic class; of scarlatina from 871 to 27; of diphtheria from 636 to 255; of croup from 229 to 90; of infantile diarrhoea from 966 to 586. There has therefore been during 1871 a remarkably great diminution in all that class of diseases. And this fact also may be an argument for their correlation. If cosmical causes produced those diseases during the worst years, they must have been of similar nature and force to causes active in other regions of the globe. In the absence of actual demonstration the argument can only be followed out by some such process of exclusion. Meanwhile, it would appear as if of all theories of causation the imported virus theory explained the phenomena with fewest assumptions.

That the epidemic the first case of which appeared in 1858 was a disease new to the colony is corroborated by the recorded observations of those who had long practised here prior to the period. In a series of papers on croup published in the *Australian Medical Journal* during 1857, the year before the outbreak, the author says:—"I have examined the morbid appearances presented after death in several cases of croup which occurred in Melbourne, and in none was there any appearance of false membrane either in the trachea or larynx." Redness, swelling, effusion and swelling, and

mucopurulent secretion, were present, but "in none of the cases have I seen any shreds or portions of false membrane expectorated." In fatal cases death took place from the mechanical impediment at the larynx from swelling of the mucous membrane, but neither from spasm nor false membrane. The same gentleman seems, singularly enough, to have met with his first case of exudative croup while engaged with his paper on the subject. The little patient recovered after expectorating the false membrane.

Viewing that case with the light since thrown upon the different forms of croup it might be asked whether it might not have been diphtheritic? It is generally believed that hitherto two essentially distinct forms have been confounded, diphtheritic croup and the laryngitic with formation of false membrane. In this colony the distinction has for at least the last ten years been observed and acted upon. But looking again at the table, and seeing the sudden rise of croup from 56 in 1857, to 102 in 1858, and 229 in 1859, the question again suggests itself if several of these cases were not mistaken. In 1860, a fall of croup cases from 229 to 156 took place, with an increase of diphtheria cases from 280 to 636, so that possibly a better diagnosis was made in the latter year. This it is now impossible to ascertain, however plausible may be the inferences from late inquiries, among which rank as important histological examination of the pellicle. But even this will not be conclusive. Merely testing the physical appearances of the detached false membrane apart from the surfaces upon which it is formed cannot give trustworthy information. For instance, the crude experiments of Küchenmeister on the solubility of diphtheritic membranes in various solvents, as lime-water, caustic potash, lactic acid, &c., cannot from their nature convey an idea of the pathology of their formation so complete as the examination of the false membrane in section while it is still in contact with the mucous surface upon which it forms. In this respect the observations of Professor Rindfleisch, of Bonn,* surpass in minuteness and accuracy any previously made, and give the basis of a true nosolo-

* Pathological Histology, 1872, p. 348, et seq.

gical distinction between two diseases in ordinary practice always separated, but recently asserted by some clinical teachers to be closely allied if not identical. It is said that membranous croup is always diphtheritic, masked it may be, or disguised. Ordinary observation saw otherwise, and now the most perfect scientific investigation yet made declares it to be correct. This will have a decided effect on the rationale of treatment. This examination "brings to light the surprising fact that psuedo-membranes do not consist of fibrine." That is to say, the pellicles in diphtheria are not simply coagulates, as they seem to be from their macroscopical appearance, but only a congeries of cells. They are found to be so when teased out under a weak ammoniacal solution of carmine, the colour pointing out the nuclei. In structure the diphtheritic pellicle differs essentially from the false membrane of true croup as commonly understood. Both membranes are it is true a result of croupous inflammation; but the quality of the psuedo-membrane varies according to the normal structure of the part on which it forms, and to the presence or absence of a general blood disease. There is thus a true distinction to be drawn between laryngo-tracheal and pharyngo-laryngeal croup. In the current clinical language, says Rindfleisch, the former is called "croup" par excellence, the latter falsely "diphtheritis," pharyngeal croup. "The physician has every reason for separating these two forms. The clinical pictures they present, the dangers with which they threaten the life of the patient, above all the treatment, are distinct in points so essential, that spite of the anatomical identity which I cannot omit proving, I would decidedly oppose a fusion of the two diseases in the clinical department." This emphatic opinion will be a decided argument against the attempt to identify the two diseases. Lately the prevailing tendency has been turning in this direction, and inclined to assert that the greater number of cases of so-called croup were in reality mistaken cases of diphtheria. But a careful perusal of the text here cited, with an inspection of the two principal illustrations of the false membranes in situ will certainly prevent the mistake and the resulting fatal confusion. However interesting, it would be too long to cite here the full description of the differ-

ences of these two exudations ; and it must suffice to state that the essential distinction consists in the diphtheritic membrane being a mass of cells applied like a plaster cast to the surface of the mucous membrane, and bound together by no fibrine, while the membrane of true croup consists of cells embedded in a matrix of fibrine. The method of separation in each case is quite different. There is a tendency to spontaneous separation by exudation of a clear mucus beneath the croupal membrane which detaches it, and so to a natural process of cure ; but in the diphtheritic exudation the cellular elements continue to exude from the diseased membrane as long as the blood disease lasts, and from the absence of the fibrine, with a tendency to a rapid putrefaction, and from that of secondary blood poisoning, a morbid state superadded to the primary malady. Hence it is that diphtheria always tends to get worse unless interfered with by treatment. It is this defect of fibrine that explains the tendency to hæmorrhage, and allies diphtheria to the zymotic diseases, such as typhus and scarlatina, and separates it from simple inflammations with plastic exudation. It is therefore still most probable that the cases of croup as recorded were not instances of misdiagnosis, the primary fauceal exudation not being overlooked, as suggested by Trousseau. Himself the greatest advocate of the identity of the diseases, he admits that rarely does the exudation in diphtheria begin in the trachea. It mostly extends into it from the pharynx. It is impossible to verify, if verification were by any one deemed requisite, the statements of Rindfleisch, for the best of reasons, that as his observations were made with the highest magnifying powers, the like observations cannot here be repeated, equally high powers not being in the colony. His account of the transit of young formative cells through pores in the homogeneous boundary layer, pores imperceptible save under the highest powers, resembles somewhat the description of the passage of blood leucocytes given by Cohnheim ; and must be accepted, and made our guide in forming a rationale of treatment as well as in solving the problem of etiology. This rationale would indicate some means of attacking not merely the secreted product, but also the secreting mucous membrane with which it is so intimately connected. Such means

may be found among diffusible agents having power to melt the aggregation of cells composing the so-called pseudo-membrane, and at the same time dialyse, as it were, and alter as far as local action alone can affect the faulty cellular metamorphoses in the parenchymatous tissues under the mucous membrane, which are indeed only the well-understood local expressions of the general pathological state. This in the diphtheritic fever, often a mere febricula, is very transient, the sources of danger being in the various consecutive processes, even in the malignant form which "pitilessly kills," and "resists all treatment."

Besides these forms of croupous inflammation, Rindfleisch speaks of the diphtheritic pock, and of diphtheritic inflammation, as seen in dysentery, wounds, hospital gangrene, &c., thus widening the application of the adjective term diphtheritic while narrowing the definition of the term diphtheria to its specific application.

Every debatable point connected with the disease has been raised as an issue here, as well as in all other places where the epidemic has raged. Its local character has been as warmly urged as it was by Bretonneau himself; and the advocates of that view still holding that constitutional symptoms solely result from secondary blood poisoning by resorption of the debris of the putrifying pellicle. They ignore the preliminary fever, and deny that there can be diphtheria without false membrane in the fauces. But all who have seen much of this trouble must have met with instances of one or two children in a family having well pronounced local signs, while several others only showed general symptoms with local hyperæmia of the mucous surfaces of the fauces. The strongest proof of the general blood affection preceding the local is found in the diminished fibrine and the consequent quality of the exudation. These two facts, now well defined, suffice to place the disease in the category of the acute blood affections, a true reproductive contagious malady. The secondary septichæmia is but an accident, similar to the putrescence in typhoid from resorption of exudative matters, or to the secondary adynamia of variola from purulent resorption; it is a true pyæmia or suppurative fever, such as may be common to many diseases only modified by the

primary affection, and commonly spoken of as the typhoid state. This state differs from the asthenia often seen in diphtheria, and arising from heart complication, or embolism; also from the muscular debility, ataxia, or paralysis, and is only one of the many constitutional states that may precede death in this protean malady.

Then again there have been the debates as to the affinity to scarlatina, many contending it to be only a form of that fever. That the two may coexist in the same house is undoubted; but every clinical fact disproves their identity. The one confers no immunity from the other. Three years ago Mr. R.'s family, five in number, had all well pronounced diphtheria at their own residence at Hawthorn. Last summer they went while all in excellent health, robust, down to Brighton to spend a few weeks of summer holiday; when in a week they were all down with scarlatina, six members of the family having it, some of them in a severe form. It was ascertained that a family from Melbourne just convalescent from scarlatina had occupied the same lodgings immediately before they went to them. This was a good illustration of the non-immunity conferred by diphtheria against scarlatina, of the infecting force of fomites and of the futility of trusting for their destruction to fumigation.

With scarlatina, albuminuria is the natural association, and this condition has also engaged attention. But the early period at which it appears in diphtheria proves it to be a primary lesion, whereas in scarlatina it is invariably secondary. No diagnostic could be more precise, and yet it is the subject of a fallacy. In diphtheria albuminuria has been suggestive of etiology, for Dr. Burdon-Sanderson infers from it that the contagium of diphtheria is of animal nature, analogous in its action to cautharidine. But this inference need not necessarily follow from the analogy, for, as Dr. Bence Jones justly remarks in his lectures on pathology, there is so little difference in the chemical composition of animal and vegetable substances, that the distinction between animal and vegetable poisons is no longer possible. Vegetable albuminous matter undergoing change may produce almost, if not quite, exactly

the same poison as animal albuminous matter. The primary albuminuria, therefore, of diphtheria need not necessarily be of animal origin.

The infectiousness of diphtheria has been another keenly disputed fact, and the doubt is in truth the basis of and apology for this paper. That it is contagious there has been almost direct experimental proof. The writer communicated it to a dog by indirect infection. He had just left a case in which he had removed the pellicle with his fingers, and without washing his hands, in a few minutes had them licked by a large mastiff in full health. On the following morning the dog was ill, in high fever, on the second day prostrate, muco-purulent matter with much frothy saliva flowing from nostrils and mouth. On the third day the bricks^r around his kennel were covered with froth and purulent sanics, the animal rapidly emaciated to a sad spectacle ; treatment was impossible from the danger of being bitten, and in the night-time between the fourth and fifth day he died. The whole fauces were coated with dark brown putrescent exudations. In this animal robust health was not proof against the specific morbid poison. It never is. Pure health and good strength are the true predisposing conditions for the typical expression of all such animal contagia. Hence often the healthiest and strongest are often first and most severely affected. Ill-health may be absolutely protective against the specific morbid poison. Sickliness often destroys the receptivity for such poisons. At any rate good health never does, except in some rare cases of idiosyncrasy. Of this, seemingly paradoxical as it is, there is furnished ample proof in our daily experiences amongst the healthy and the sick.

Of this infectiousness of diphtheria there has just been most seasonably for an argument supplied a convincing proof. The following extract from *The Argus* of 22nd July, 1872, explains this :—

“During the past two or three months Port Lincoln, one of the most healthy districts in South Australia, has been suffering terribly from another epidemic — diphtheria. After the juvenile population had been well-nigh decimated, the Government sent

over Dr. Phillips from Adelaide to ascertain what could be done to stay the ravages of the disease. His report has recently been published, and forms a valuable contribution in favour of the theory—if theory it can be called—that diphtheria is an exceedingly infectious disorder. He traced its introduction into the district to a family that had come overland from the Darling, and followed it from house to house, showing with convincing precision that in not a single instance had it come spontaneously, but that one infected child had contaminated another. The disease has now abated, partly through having exhausted its victims, and partly as the result of the remedies applied under Dr. Phillips's directions."

This report of Dr. Phillips gives the *coup de grâce* to the spontaneous origin theory as far as Australia is concerned. It is not too much to assert that, had that family from the Darling been isolated, as they would assuredly have been had they been afflicted with small-pox, the fever would not have spread among the rest of the population. From that also it may equally safely be averred that, had the first case of diphtheria in Melbourne been isolated and as rigorously quarantined as the recent case of small-pox at Sandhurst, it would have been as energetically stamped out. But isolation has never been thought of for any contagious fever save small-pox ; hence their spread, and the loose assertions of their continual spontaneous origin. Their suppression has been expected by the use of disinfectants, but notwithstanding free use of these, the fevers still prevail and spread. They would have been as unavailing against small-pox or aphtha, without isolation in the one case, and destruction of the affected cattle in the other. An insular position saved the Scilly Isles for ten years from a single case of small-pox, scarlet fever, measles, or diphtheria. An insular position preserved the Faroe Islands for sixty-five years. A cordon would have saved Port Lincoln. Our insular position should protect us. It will from cholera or yellow-fever, or cattle typhus ; but certainly not by trifling with disinfectants and a too facile pratique. As yellow-fever was taken to St. Nazaire, by men unloading a ship from Havannah, so may it come to us. We have been compared to Buenos Ayres before the epidemic was taken there. They trusted to their fine

climate for immunity, till specific contagion broke their dream of security.

To show how morbid germs or matter may remain in a latent state, external to all organic life, to scatter after a time with equal severity, and independent of any new contagion from without, the germs remaining concealed in hangings and other furniture, Trousseau relates the following case, the counterpart of which could easily be found in Victoria. "A girl of nine years of age was carried off by malignant diphtheria. On the first manifestation of the symptoms her two sisters were removed to a distance from the house, and did not take the disease. But eight months afterwards, on returning home, the elder of the two was seized with diphtheria, which invaded the larynx; and I was called in to perform tracheotomy. This child died, as her sister had died, from diphtheritic poisoning. Again, on this occasion, as soon as the disease was recognised, the surviving sister, aged five years, was sent off to the residence of her grandmother; but she carried with her the germ of the malady. Sore throat very soon declared itself, croup necessitated tracheotomy, which in this case was successful. Two circumstances in the history of these children require to be looked at separately; viz., the preservation of the germ, external to the organism; and the incubation of the malady. * * * The period of incubation is, at least in some diseases, as you know, longer or shorter in different persons according to their individual peculiarities."

Although it is no longer the rule to bow implicitly to authority in matters of medical opinion, yet there are men to whose expressed convictions the greatest deference is deservedly due, and will be as long as wisdom is held worthy of respect. In the latest edition, the sixth, of Dr. Aitken's treatise, 1872, vol. i. p. 367, it is said:—

"The belief in the spontaneous endemic origin of the specific miasmatic diseases rests on evidence entirely *negative*. * * * The existence of the specific disease poisons is known to us by inference only. * * Cases thus constantly arise which appear to give countenance to the belief that the disease has had a spontaneous origin. * * * No common conditions of human life gave rise to such phenomena. Propagation from the actual poison of a pre-existing

case was the one necessary and all-sufficient condition for their endemic outbreak, and their epidemic prevalence. * * * How they first came into existence is beyond our ken. * * * Once created, they all propagate only in one way—namely by continuous succession. * * * And so any of these diseases would thus have all the appearance of having arisen *de novo*.”

So likewise in the latest edition of Sir Thomas Watson's Lectures, the fifth, 1871, vol. ii. p. 844, that famous clinical teacher sums up his investigations of this subject with the statement that “The history of small-pox leads to the settled belief that this disorder never occurs except from contagion. * * * The same thing may happen in *other* contagious diseases, of which the contagious property may not be so strong or obvious. * * * If once a disorder of this kind is clearly proved to be sometimes the effect of contagion, we cannot help entertaining a doubt whether the disorder in question really has ever in reality any other cause.”

Again, Trousseau, vol. iii. p. 23, Syd. Soc. Trans., declaring his belief in a spontaneous origin of the *earliest* germs, says, “That period is very remote from the present time, and since that time, whenever it may have been, they have always, as now, been reproduced by contagion. * * * Morbific germs may remain inactive for a certain time, adherent to inorganic bodies, * * * hidden for days, months, or years, waiting to manifest their presence, till they meet with conditions favorable for their isolation.” * * * One drop of water suffices to bring about the resurrection.”

To the same end the Sixth Report to The Privy Council, On Public Health, by John Simon, Esq., might be referred to for a most instructive and dispassionate discussion of this subject; as well as the writings of many of our ablest and greatest living teachers. The discussion now going on among scientific men on this subject of contagion and disease-germs, determines as yet nothing in a clinical direction. When Pasteur and Bastian, Beale and Pouchet, Hallier and Sanderson, Chauvaux and Lemaire, and other equally distinguished names in this greatest of controversies, cannot verify one another's observations about corpuscles or specks,

ranging from a 1,200,000th, to the 1,100,000th of an inch in diameter, it would, to say the least, be hazardous to venture into a theoretical practice based on either anticipated conclusion. The origination of the first germ is a question out of the range of science, as Niemeyer justly says. It is enough for us to know how the last germ is propagated. This is the problem for physicians to decide in this life. The other may perhaps be resolved by the metaphysicians in the next.

As it is with the natural history of diphtheria, so it is with the proofs of identity derivable from the conflicting varieties of treatment. Here, as elsewhere, local and general remedies must be resorted to, and occasionally the most trusted of them lamentably fail. There may be said what has been said in other countries, that he who boasts of never having had an untoward result of treatment, never had a formidable case of true diphtheria to treat. It is indeed freely admitted on all hands that climate has not modified the disease in the least degree, and that in every variety of Australasian climate the specific nature is the same. The local and general symptoms; the general pathognomonic symptoms; the accidental or incidental symptoms; their succession; the complications, and varieties; the varied sources of danger; the prodromata, and sequelæ, are always of the same, now trivial now intractable kind. As already said, the self-same discussion as to the local or general character of the pathology has here been gone over again with careful iteration but without adding the scintilla of a novel idea on the subject. The same topical applications have proved useful and useless. Some have a panacea, others are without resource of cure, and trust to nature and nourishment.

The nitrate of silver, solid and in solution, strong or weak; alum and tannin, as used a thousand years ago by Aræteus, and as rediscovered by the old French lady, who first cured her pigs with it; and then told the secret to Trousseau and her priest; borax; soda chlorate; lime water; manganese; ice; muriatic acid; pepsine; bromine; styptic ether; perhaps even the actual cautery; sulphurous acid; muriate of iron; iodine; carbolic acid; sulphocarbulates; and many other remedial agents have been vaunted as

more or less valuable, not even omitting snake-root, and poke-weed, guaiacum and ozone. So vast an array of remedies is suggestive of the old adage that two classes of diseases, the very slight and the very severe, are alike provided for by a great variety of medicines.

To claim for the profession justice, few if any of the means of the general or special indications of treatment have had asserted for them any specific virtues. Every one of the many trusted remedies is used with a rational design of counteracting some one or other of the modes in which a tendency to death may show itself in a disease, which terminates fatally in a greater variety of ways than perhaps any other malady. To this extent only, is any form of rational treatment held to be specific. But, as the disease, and its treatment have been found here, so have they been experienced in other countries ; proving that no peculiar but a common causation was at work, which is most probably, a specific and easily transportable contagium.

Happily for all, the statistics inform us how greatly the epidemic is on the wane. Another year of the same ratio of diminution and it would entirely disappear. As in all epidemics, towards the close, the severity abates, although exceptionable cases of extreme severity must still often occur. Still in the main, the pith is lessened, and the seeming potency of remedies increases. This has always been a source of fallacy of observation ; and we shall be wise to be upon our guard against being so misled.

"ORIGIN OF INFECTIOUS DISEASES."

"It is all but hopeless to expect any satisfactory proof of the independent origin of infectious diseases in densely crowded communities in which the majority of cases may be traced to direct contagion. We may suspect, for example, that typhus may originate in rooms densely overcrowded by poor and starving people; but it would still be difficult to show that the fever poison might not have been introduced from without. So also we may be able to prove to our satisfaction that the admission of sewer gas into a bedroom, or the drinking of water impregnated with the same, gives rise to typhoid fever; but it will be difficult to show that the sewage has not been poisoned by typhoid discharges, and the medium by which infection has been carried. The proof of the independent origin of scarlet fever or small-pox would be still more difficult, because the cases in which the course of the infection could be traced would be proportionally much more numerous. But these difficulties are greatly lessened in new and sparsely populated districts, and medical men practising in the colonies would render an essential service to science if they would carefully investigate the first appearance of zymotic diseases in isolated houses. We are glad, therefore, to notice that the subject is attracting their attention; and an interesting letter has just appeared in the *Melbourne Argus* urging the necessity of an exact investigation in each case. The writer states that the first death from diphtheria was registered on Oct. 2nd, 1858, in the neighbourhood of West Melbourne, and, singularly enough, it was recorded by the same medical man who had registered the first deaths of two outbreaks of epidemic fever. All these deaths occurred in the neighbourhood of a swamp, close to which are streets not protected by trees. It is under such conditions alone that we may expect to obtain satisfactory proof of the spontaneous origin of infectious poisons, and we shall be glad to receive information from such sources."—*Lancet*, May 4, 1872.

